

last discovery has important bearings in other directions. In the vertebral column there appear to be a total of 76 vertebrae in the case of *H. horridum* to but 63 or 65 in the spine of *H. suspectum*. And, in conclusion, this distinguished herpetologist remarks that "A short rib is present on the third cervical in *H. horridum*, which is absent in *H. suspectum*; the neural spines are more elevated in the middle and posterior portion of the dorsal region in *H. horridum*, specimens of the same sex, of course, being compared. The neural spines are much more developed in the male" (p. 116). Boulenger still thinks the place of the *Helodermatidae* between the *Anguillae* and the *Varamidae*, which he assigned to them in 1884.

In concluding this notice I am led to pass some observations upon certain strictures Mr. Boulenger has made in his paper upon my memoir on the anatomy of *H. suspectum* which appeared in the Proc. of the Zool. Soc. of London for 1890. His criticism of my description of the atlas of *H. suspectum* is well taken, as I have satisfied myself of by an examination of better material since. That bone is found to be in three pieces, and not in five as stated by me. He is also quite correct when he comes to point out certain errors in my figures of *manus* and *pes* of that reptile, and I thank him for having called my attention to them. With respect, however, to the error he believes me to be guilty of in my description of the teeth in the premaxillary bones of *H. suspectum*, I can in no way agree with him. He observes: "Eight or nine præmaxillary teeth are present in *H. horridum*, and only six in *H. suspectum*. Dr. Shufeldt, however, represents eight teeth in the latter species; but his figure, showing all the teeth as of the same size, looks very diagrammatic." In his figures illustrating these remarks Mr. Boulenger gives *H. horridum* but six teeth, and *H. suspectum* but four, and the drawings of the bones look, indeed, very diagrammatic. I cannot conceive of any lizard normally having but "nine" teeth in its premaxillary bone; it should at least be an even number. Now the mounted specimen of *H. suspectum* in the collections of the U.S. National Museum, has eight teeth in its premaxillary, and it was from that specimen that I drew my figure which appeared in the Proceedings. Normally, that is the number, but those teeth are often broken out in the *Heloderms*, and they become irregular by subsequent growth. The outer ones are always the longer, when the skull is perfect. In so far as the form of the premaxillary is concerned, as touched upon by Mr. Boulenger, I believe no little allowance must be made for individual variation, which is often quite considerable among lizards as it is among Vertebrates higher in the scale. Other figures illustrating the work under consideration are excellent.

It would appear that it is to be the fate of the *Helodermatoidae* to have their morphology more thoroughly worked out than most, or even any other, lizards up to the present time; and I am given to understand that Prof. Garman, of Harvard College, has it in mind to review, in the near future, the entire structure of *H. suspectum*.

R. W. SHUFELDT.

Smithsonian Institution, July 8.

THE DISCOVERY OF THE STANDARDS OF 1758.

THE discovery by the Clerk of the Journals of the House of Commons, referred to in NATURE last week (p. 280), of the original standards of length, which were in 1758 deposited in the custody of the Clerk of the House, has attracted some attention to the history of these Parliamentary standards. As some misapprehension as to the effect of such discovery appears to have arisen, and as it is to eminent men of science that we are mainly indebted for our standards of length, the following explanatory notes may interest many of our readers.

The standards of length above referred to were made under the directions of a Committee of the House of Commons, of which Lord Carysfort was Chairman, appointed on May 26, 1758, "to inquire into the original standards of weights and measures in this kingdom." The Committee reported that in 1742 several members of the Royal Society were at great pains in taking an exact measure of the ancient Exchequer standards of length (of

Henry VII. and Queen Elizabeth), then condemned by the Committee as being coarsely made and "bad standards"; that such measure was made by "very curious instruments prepared by the late ingenious Mr. Graham"; and that the Royal Society had lent to the Committee a brass rod made pursuant to these experiments, which rod had been compared by Mr. Harris, of the Royal Mint, with the Exchequer standards. Mr. Harris advised the Committee that the Royal Society's standard was made so accurately, and by persons so skilful and exact, that he did not think it easy to obtain a better standard; and accordingly the Committee then had two rods made by Mr. Bird, an optician, according to Mr. Harris's proposal; which "rods" were laid before the House. The rod marked "Standard Yard, 1758," was to be taken as the proper standard; it was stated by the Committee to be made of brass, to be about 38 or 39 inches in length and about one inch thick; near to each end of the rod a fine point and line being drawn on a gold stud, the distance between the points on the gold studs being the "true standard length of a yard," or 36 inches. The second rod was made in the same manner as the first rod, excepting that it had "two upright cheeks" instead of points or lines; so that any other yard rod might be measured by being placed between the cheeks. Both these rods (together with three standard troy pounds

marked "T," with a crown and "G.2," and a set of troy

standards from 2 pounds to 32 pounds, made and adjusted by Mr. Harris "with very curious and exact scales of his at the Mint") were stated by the Committee to be then deposited with the Clerk of the House of Commons.

In 1838 the attention of the Government was directed to the necessity of determining a new standard weight and measure to replace the above standards of 1758, which were stated by the Chancellor of the Exchequer—in a letter to Mr. G. Airy, the Astronomer-Royal—to have been "destroyed by the burning of the Houses of Parliament," and a Commission was appointed to restore the standards. The Commission included F. Baily, J. E. D. Bethune, Davies Gilbert, J. S. Lefevre, J. W. Lubbock, Geo. Peacock, R. Sheepshanks, J. Herschel, and G. B. Airy. Their report of 1841 gives a precise description of the condition of the standards at the Journal Office immediately after the fire. The Committee reported that the legal standard of one yard was "so far injured that it was impossible to ascertain from it with the most moderate accuracy the statutable length of one yard"; and also that the "legal standard of one troy pound was missing." New Parliamentary standards of length and weight were accordingly made under the directions of the Committee, and were legalized by an Act of Parliament passed in 1855. These new Imperial standards are now deposited with the Board of Trade, but legal "Parliamentary copies" of them are stated to have been immured, in 1853, in the House of Commons, and further copies were then deposited at the Royal Mint, the Royal Observatory, and with the Royal Society. These latter Parliamentary copies are legally required to be compared with each other once in every ten years, but those deposited at the House of Commons are excepted from any such comparison. It would appear, however, that the House of Commons standards are sometimes examined, as is shown by some printed correspondence on this subject which was laid before the House of Lords in 1872, in which year the standards were examined, and after their examination were again immured in a wall near the lower waiting hall of the House of Commons; a certificate of the deposit of the standards being given as follows:—

"It is hereby certified that this day, in the presence of the undersigned, the oaken box containing the Parliamentary Copy No. 4 of the Imperial Standard Yard, and the Imperial Copy No. 4 of the Imperial Standard Pound, . . . has been "deposited within the wall on

the right-hand side of the second landing of the public staircase leading from the lower waiting hall up to the Commons Committee Rooms, a brass plate having been fixed upon the wall bearing the following inscription in Elizabethan or church text :—Within this wall are deposited standards of the British Yard Measure and the British Pound Weight, 1853." The certificate is signed by G. B. Airy (Astronomer-Royal), John George Shaw Lefevre (Clerk of the Parliaments), W. H. Miller, C. P. Fortescue (President of the Board of Trade), H. W. Chisholm, and H. J. Chaney; and is dated March 7, 1872.

It hardly appears, therefore, that the old standards of 1758, which appear to have remained unnoticed for the past fifty years, are now of any importance for the purposes of measurement.

MAXWELL'S ELECTRO-MAGNETIC THEORIES.¹

AN account of Maxwell's electric theories from the pen of Prof. Poincaré could not but be full of interest. The volume before us is the first of two on the views and conclusions set forth in the "Electricity and Magnetism" regarding electro-static and electro-magnetic action, and their verification by Hertz and others; and we must of course wait for the completion of the work before we can form any adequate idea of its scope and character, and fully understand the results of the critical analysis which it contains. But in spite of the fact that the treatise is in the somewhat disadvantageous form of an edited course of lectures, it is a contribution of great value to the literature of the subject. Whether or not it is possible always to agree with the physical views expressed regarding matters which are not yet outside the region of speculation, it is impossible not to admire its style and methods. Here are to be found exemplified that order and harmony which render the work of the best French mathematical writers so exquisitely clear, and that artistic charm which is so seldom seen in the writings of scientific men of other nationalities. It has been remarked by competent critics that Maxwell's work, though essentially that of an artist and man of genius, is obscured here and there by a certain vagueness and want of logical coherence and completeness, which has tried the patience and strength of many a devoted disciple. This was of course to a great extent inevitable. He sought out new fields of speculation for himself, and his greatest and most successful generalizations were, one cannot help feeling, the results rather of unerring intuition than of any completely systematic process of reasoning. Those who follow in his footsteps therefore are glad of the help of any friendly guide who is able by his experience and strength to point out the dangers and diminish the difficulties which attend their progress.

In his introduction Prof. Poincaré gives a critical estimate of Maxwell's theories which strikes one at first sight as somewhat inappreciative. Thus he says :—

"La première fois qu'un lecteur français ouvre le livre de Maxwell, un sentiment de malaise, et souvent même de défiance se mêle d'abord à son admiration. Ce n'est qu'après un commerce prolongé et au prix de beaucoup d'efforts, que ce sentiment se dissipe. Quelques esprits éminents le conservent même toujours. . . . Ainsi en ouvrant Maxwell un Français s'attend à y trouver un ensemble théorique aussi logique et aussi précis que l'optique physique fondée sur l'hypothèse de l'éther; il se prépare ainsi une déception que je voudrais éviter au lecteur en l'avertissant tout de suite de ce qu'il doit chercher dans Maxwell et de ce qu'il n'y saurait trouver.

¹ "Électricité et Optique." I. Les Théories de Maxwell et la Théorie Électromagnétique de la Lumière. Par H. Poincaré, Membre de l'Institut. (Paris : Georges Carré, 1890.)

"Maxwell ne donne pas une explication mécanique de l'électricité et du magnétisme; il se borne à démontrer que cette explication est possible.

"Il montre également que les phénomènes optiques ne sont qu'un cas particulier des phénomènes électromagnétiques. De toute théorie de l'électricité on pourra donc déduire immédiatement une théorie de la lumière.

"La réciproque n'est malheureusement pas vraie; d'une explication complète de la lumière, il n'est pas toujours aisé de tirer une explication complète des phénomènes électriques."

The author, however, shows throughout his exposition that he is not only impressed with the extraordinary importance of Maxwell's work, but also thoroughly appreciates and admires, if occasionally under protest and with longing after the more ancient classic models, its somewhat wild and native beauty.

An important part of the introduction is an exposition of the theoretical basis of what Prof. Poincaré rightly regards as the fundamental idea of Maxwell's treatment of electro-magnetism—that is, the application of the general processes of dynamics to any system of current-carrying conductors. No doubt almost all the work which had been done previously had been more or less of this nature, but we refer here to the attempt which Maxwell made with very considerable success to correlate electro-magnetic phenomena by means of Lagrange's general dynamical equations.

In the Lagrangian method the physical state of a system is defined by means of certain parameters $q_1, q_2, \dots, q_n$, n in number; and a dynamical explanation is obtained, or proved to be possible, when the values of these parameters are found in terms of, or proved to be related to the positions and motions of a system of connected particles, either of ordinary matter, or of some hypothetical fluid.

If $m_1, m_2, \dots, m_p$ be the masses of these particles, x_i, y_i, z_i the Cartesian co-ordinates of the particle of mass m_i , and if the system have potential energy V , a function of the $3p$ co-ordinates of type x_i, y_i, z_i , there are $3p$ equations of motion of the form

$$\left. \begin{array}{l} m_i \ddot{x}_i + dV/dx_i = 0 \\ \text{\&c.} \quad \text{\&c.} \end{array} \right\} \dots \dots (1)$$

The kinetic energy T is

$$\frac{1}{2} \sum m (\dot{x}^2 + \dot{y}^2 + \dot{z}^2),$$

and the principle of conservation of energy gives $T + V = \text{constant}$.

Now we know V , and can express the co-ordinates of each particle or molecule in terms of the n parameters $q_1, q_2, \dots, q_n$. The celebrated Lagrangian equations in terms of the parameters can then be obtained by direct transformation of (1), and are of the type

$$\frac{d}{dt} \frac{\partial T}{\partial \dot{q}_k} - \frac{\partial T}{\partial q_k} + \frac{\partial V}{\partial q_k} = 0.$$

Here T and V are homogeneous quadratic functions, the first of the quantities of type $\dot{q}$, with coefficients which are functions of the parameters themselves, the latter of the parameters only.

If we have reason to believe that the system we are dealing with is a dynamical system, for which the values of T and V (or, more properly, those parts of the total kinetic and potential energies which are concerned in the special phenomenon treated), can be obtained by observation of parameters of type q , we can use these equations in our discussions of results, whether or not we can actually express the parameters in terms of co-ordinates of particles of the system. The justification of this process is the agreement of the results with experiment.

If now we imagine a system of particles (whether of